THE MINNESOTA ACADEMY OF NATURAL SCIENCES.

THE April meeting of the Academy, which proved to be a very interesting one, was addressed by Mr. F. K. Butters, who spoke on the 'Fungus Flora of Minnesota.' The more fundamental relationships of the fungi were illustrated by slides, taken from Engler and Prantl's 'Pflanzenfamilien,' and also by original microphotographs of common minute Phycomycetes and Ascomycetes, as well as by a large number of photographs of the fleshy fungi of the locality, taken in the field to show their natural conditions of growth.

The speaker pointed out that the fungi are to be considered as one of the most successful groups of the plants, showed by their diversity of form, the great variety of conditions under which they will grow and their numerical importance. In Minnesota the number of species is probably in excess of the flowering plants. The diverse conditions under which fungi will grow, *e. g.*, in water, upon living plants and animals, on decaying organic substances, in humus and in sand containing a minimum amount of organic matter was illustrated. Attention was called to the fact that on account of the great number of spores produced wherever there is suitable environment there also will be numbers of plants. The diverse forms of fungi are modifications of a few types to be regarded as distinct phylla and parallel lines of development sometimes exist in different groups.

The lecture was productive of a very general discussion of local fungi and was greatly enjoyed by the large audience present.

F. G. WARVELLE.

DISCUSSION AND CORRESPONDENCE.

GEOLOGY OF CHINA.

TO THE EDITOR OF SCIENCE: In the discussion in the last number of SCIENCE of my article in *McClure's Magazine*, there are some things which deserve attention in order to get the facts fairly before the public.

1. It is proper for me to state that the title of the paper and the headlines were put in by the editors; so that I was in no sense responsible for them. I think that in the article itself there are no offensive claims to original discovery.

2. The quotation from Geikie's 'Great Ice Age' (p. 699) is unfortunate for my critic, since it was that very quotation which misled me during a considerable portion of my trip. In this quotation Geikie says, "Its materials [those of the loess], we may believe, are largely of *fluvio-glacial* origin, and represent in great measure the flood-loams swept down from the mountains and plateaux when these supported extensive snow-fields and glaciers. But, as Baron Richthofen in his great book on China has demonstrated, the loess, as we now see it, owes its structure and heaping up to the action of the wind, and is even now forming and accumulating in many regions of Asia. It is, in short, a true steppe-formation." On page 697 Geikie had said, "According to Przevalski, undoubted traces of former glaciation are seen in the Suma-Hada range, west of Kalgan in China." My first point was to visit this mountainous region west of Kalgan supposed both by the Russian and by Geikie to be the source of the loess in Eastern China. But we found no indications of glaciation in that region, and pursued our investigations sufficiently to convince us that there were none; so that Geikie's theory of the 'fluvio-glacial origin' of the loess falls to the ground in that region. That came pretty near being a discovery.

3. On the same page Geikie says, "Kropotkin's researches have led him to conclude that the whole of the upper plateau of Asia and its border-ridges were under a mighty ice-cap." Assuming the truth of these statements, Geikie says upon the next page, "The mountain-valleys everywhere contain wide and thick sheets of rounded blocks, and coarse and fine gravel, which are in every respect comparable to the fluvio-glacial gravels of the Alps. But in none of the descriptions of these which I have read is there any clear indication as to whether the deposits occur in successive terraces like the high- and low-level terrace gravels of the Alpine lands of Europe. Something like this arrangement seems to be present in the valleys of the Thian Shan, and may possibly refer to recurrent phases of glaciation." In accordance

with this view, the glacial map of Asia which precedes the chapter is covered with extensive glaciated areas over the regions which I have specifically visited. All of which shows the confusion of mind which has widely prevailed up to the present time concerning the glacial conditions of Southern Siberia and Central Asia, and goes to justify the editor in naming my article.

4. I am not aware that Kropotkin had any personal knowledge of the southeastern border of West Turkestan. But it is significant that Geikie, on his authority, speaks of 'immense sheets and terraces of loess' fringing the base of its mountainous border. The writer in SCIENCE assumes, as I believe unwarrantably, that the only indication of a former sea-level is the occurrence of sea shells. On the contrary, in the broader studies of physical geography that are now current, sea-levels may be determined in many places by terraces where shells are not present.

5. With reference to the occurrence of the bones of land animals and of terrestrial mollusks in the loess, I need only to say, that the great uncertainty concerning the situation of these remains with reference to the original deposit largely, if it does not entirely, breaks the force of the argument which is drawn from it. No one will deny that the wind has in many instances redeposited vast amounts of loess, nor that subsequent streams have done the same. But to go no farther than our own country, it is difficult for any one who is familiar with the situation of the loess over Northern Missouri, for instance, or in the center of the Mississippi Valley at Vicksburg, to believe that it has been deposited either by the wind or by flowing streams of water when the land stood at its present level.

In due time I hope to bring the facts in fuller detail before the public. But this much I may confidently say, that the whole problem of the loess has not yet been fully comprehended, much less has it been solved. If the renewed discussion elicited by my report shall contribute to an understanding of the subject, a great point will be gained. But I am sure that the as yet little understood facts of Central Asia will contribute much toward a solution of what has been one of the most perplexing of all the geological problems.

G. FREDERICK WRIGHT.

'THE LARYNX AS AN INSTRUMENT OF MUSIC.'

TO THE EDITOR OF SCIENCE: Noting in your issue of May 24, a communication from Arthur Gordon Webster quoting Professor Le Conte's reference to the larnyx, comparing it in its function to a horn and citing a passage from Helmholtz containing the same comparison, I am tempted to refer your readers to a much earlier example of the same conception. I quote the following from some notes of mine: "In 1700 Dodart ('Memoire sur les causes de la voix de l'homme,' par M. Dodart, *Memoire de l'Academie des Sciences*, 1700, p. 238) insisted that the trachea only furnishes the material of the voice, *i. e.*, the expired air. The glottis is the only organ of the voice. All the effects of the glottis for tones depend on the tension of its lips, and of its various internal structures. The concavity of the mouth has no part in the production of the voice, but it is a modifier of it, and still more is this true of the nose. He showed that Galen's comparison to a flute could not be accepted if one were to go into details. He spoke of the vibration of the ligaments, of the dilatations and contractions of the glottis. He asserted that the trachea is elongated in high notes and shortened in low ones. He likened the vocal organ rather *to a horn or trumpet. According to him the glottis is the place which corresponds to the lips of the musician; the body of the instrument extends from the glottis to the external orifice of the vocal canal, that is to say the mouth.*"

JONATHAN WRIGHT.

SHORTER ARTICLES.

PREDETERMINED ROOT-HAIR CELLS IN AZOLLA AND OTHER PLANTS.

ORDINARY root-hairs arise in acropetal succession in the zone where the surface tissue is becoming fixed; that is to say, in a region at some distance from the root apex, where the cells have ceased to divide and have reached, or are reaching, full elongation. They come from any or all of the superficial cells indif-